

25 January 1960

Professor Leo Szilard

Dear Leo,

Dr. Howard Green has been kind enough to send me the copies of your two new papers, as you had requested, and I have read them with much interest.

I have never made a thorough study of the problem of induction of enzyme formation, and for this reason it is not possible for me to speak very significantly about your first paper. I agree with you that it is possible that an organism might have such a nature as to permit, for a particular enzyme, the enzyme level to be either low or high, and I think that it is well worth while for you to have made your analysis of the problem, permitting you to reach this conclusion.

On the other hand, I remain pretty skeptical about the application of this idea to antibody formation, although something of this sort is without doubt involved in the manufacture of gamma globulin or other antibody precursors. I think that experimental results obtained during the last twenty years have not changed the situation very much from that which existed in 1940, the time of publication of my paper on a theory of the structure of antibodies and the nature of serological reactions. I still feel that the antigen molecule or a portion of it containing the hapten serves as the template for the combining regions of the antibody molecules, and that special enzymes or genes are not involved in the manufacture of different antibodies. The ability of the rabbit to manufacture antibodies against a haptenic group seems to be determined by the chemical nature of the haptenic group, and not to show the hit-or-miss character that might be expected in case that different genes and enzymes were required for different antigens. Moreover, it seems to me inexplicable on the basis of the non-template idea that animals should be able to manufacture highly specific antibodies against haptenic groups that do not occur in nature, and that do not resemble closely molecules that occur in nature. Moreover, if different genes are involved in the manufacture of different antibodies, we would expect that the amino-acid composition and sequence for the different antibodies would in general be different, whereas in fact they are found to be the same, to the extent that they have been investigated.

I have published about a dozen papers on antibodies, mainly the discussion of experimental work carried out in our laboratory, but the reprints are exhausted. I have, however, found an extra copy of my first paper, which I enclose.

With best regards, I am

Sincerely yours,

Linus Pauling :jh

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P.S. Please let me know what you think about the arguments that I have given above, in particular, the one about constancy of amino-acid composition and sequence.

Ava Helen joins me in sending best wishes to you.

L. P.